

CHECKLISTS, SYSTEMATICS AND THE CLECOM INITIATIVE: AN ALTERNATIVE VIEW FROM EUROPE

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INTRODUCTION

Davis (2004) raised important issues about the function and construction of species checklists, and, by way of examples, provided a critique of two recent lists produced in Europe: the CLECOM I list for the land and freshwater Mollusca of northern, Atlantic and central Europe (Falkner et al., 2001), and the more detailed list for France alone (Bouchet, 2002; Falkner et al., 2002). It may be noted that CLECOM I is accompanied by CLECOM sections I and II (Bank et al., 2001), which provides a supraspecific classification for a larger area, including the whole of Europe, Turkey and Macaronesia.

His critique provoked two responses from the authors (Bouchet, 2006; Bank et al., 2006). In the first of these, some differences of opinion are thought to reflect the impact of real differences in the degree and character of taxonomic differentiation in regional faunas on thought and practice, while in the second the authors suggest that Davis is privileging molecular taxonomy at the expense of the use of morphological characters. The reference by Davis (2004) to the notorious “Nouvelle École” of Bourguignat and his followers (Dance, 1970), in particular, is seen as provocative and unjustified. We note that a significant part of these arguments relate as much to the principles and procedures of classification as to the function and content of checklists as such.

Both Davis and his respondents raise general and more specific issues; the latter are mainly concerned with details of work on particular freshwater groups. As workers mainly on terrestrial molluscs, we do not intend to enter into those debates. We believe, however, that the general issues are of significance to the malacological community, and there is a danger that arguments about such lists could become very parochial, reinforcing the rather negative image that tarnishes taxonomists generally. At the extreme, they could provide

ammunition to those wishing to overturn the whole Linnean system (Godfray & Knapp, 2004).

We believe that these checklists do indeed raise issues of general importance, and we attempt to identify and resolve them. While one of us (RADC) is both a user, whose tyranny Bouchet (2006) suspects, and also an Anglophone, believed to belong to a conservative group with “lumping” tendencies (criticisms made in both responses), we hope that our combined, international contribution will be seen as constructive.

THE EUROPEAN FAUNA AND THE CLECOM INITIATIVE

Before any direct engagement with the arguments, we should acknowledge that the compilation of a European checklist presents particular problems. Proportionately, there are far more early descriptions of species than in any other part of the world; these are often inadequate by modern standards. Language problems, and the limited circulation of many publications, have resulted in a considerable amount of accidental synonymy, aggravated by some extreme schools of taxonomic practice (e.g., the “Nouvelle École” referred to above). A variety of rival higher classifications exist; some, apparently adopted completely by CLECOM, are far from universally known or accepted (e.g., Shileyko, 1978, 1984). National checklists reflect local traditions and ways of thinking, and different names for the same species may co-exist in the literature for many decades. Hence, the CLECOM initiative, endorsed by Unitas Malacologica in 1995 (Bank et al., 2006), represents a brave and arduous attempt to achieve coherence and uniformity. It is clear, both from the account of procedure given in the original (Falkner et al., 2001) and in the response to Davis (Bank et al., 2006), that much time and effort was spent not only

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in critical examination of literature, but also in finding and examining types. Nor has there been a slavish adherence to priority; individual authors of the CLECOM lists have sought and obtained ICZN rulings suppressing the earliest, but scarcely used names for some species. Nevertheless, we find ourselves in agreement with many of Davis' concerns, and we have some others that we believe should be expressed openly.

CHECKLISTS AND CLASSIFICATION

Bouchet (2006) heads one section of his response "Regional species checklists: what they are not." In this, he agrees with Davis, and with us, that they are not appropriate vehicles for advancing novel phylogenetic hypotheses, and that "elaborate, finely dissected classifications" are unnecessary. CLECOM I, however, presents us with such a finely dissected classification, and, through the nomenclature involved, also accepts, without published references, phylogenetic hypotheses that are by no means universally accepted. A checklist is not the kind of publication in which taxonomic decisions are made, and should contain references to publications in which the decisions adopted were made and supported by evidence. In their replies to Davis, the authors frequently explain in detail why they have used particular names and classifications. Such explanations would be superfluous if the information was accessible through reference to the detailed monographs or descriptions used.

We acknowledge that this task of documentation, for such a large fauna, is a daunting one. It is easier for smaller regions, as shown in the last authoritative British checklist (Waldén, 1976), in which the new list is supported by notes explaining changes from previous lists, and gives details of ICZN opinions and lists in each case. Bank et al. (2006) comment on Cameron's (2003) use of the name *Oxyloma pfeifferi* (Rossmässler, 1835), rather than the earlier *O. elegans* (Risso, 1826), as an example of traditionalism. In the context, however, it was used on the basis of Waldén's explicit argument, to retain consistency. There is now no doubt, and a new British checklist (Anderson, 2005) acknowledges, that *elegans* is indeed the correct epithet; in the meantime, a secondary publication (an identification guide for British users) stuck to the locally accepted name, but also drew attention to the use of *elegans* elsewhere and in the CLECOM list.

Similarly, the authoritative account of Polish land molluscs includes synonyms and taxonomic notes (Riedel, 1988). The CLECOM list offers no synonyms to assist users.

Furthermore, some entries in the CLECOM list evidently rest on the unpublished conclusions of the authors. In a few cases, this results from the examination of types, and of new material from type localities, originally described by members of the "Nouvelle École", no doubt accounting for the reference in the title of Davis' paper. Such entries sometimes revive old species names later synonymised with others. For example, in species in the genus *Euconulus*, old names are rescued from synonymy, and the number of species increased, on the basis of work in preparation that has not yet appeared. In this case, the fact that work is in preparation is made known not in the CLECOM I list itself, but in the list for the French fauna (Falkner et al., 2002), which is annotated. Those consulting the CLECOM I list alone have no guidance, nor can they tell what has happened to the generally recognised *E. alderi* (Gray, 1840), which disappears without trace. We do not doubt that such examinations were thorough; the conclusions may, indeed, be correct. The problem is that the evidence is not visible. Those in doubt about a particular identification are left to chase round museums to inspect the types for themselves, and, indeed, may remain unsure from which currently recognised species the segregation has been made. We note that the French list (Falkner et al., 2002) is annotated, and avoids some of these problems.

Put simply, checklists are a product of systematics, not substitutes for it. While it is true that traditional taxonomic revisions have become unfashionable (Wheeler, 2004), checklists depend on such revising monographs and on new descriptions or redescriptions of species. These tasks are hard, but not impossible; we would cite, for example, the work of Alan Solem on endodontoid snails of Pacific islands and camaenid snails in Australia, encompassing descriptions or revisions of over 600 species (Cameron et al., 2005). As Bouchet (2006) says "... checklists are only as good as the quality of the science behind them", a point also made by Davis. Of course, some science may be both good and wrong; more information, or new analytical techniques may alter conclusions. Changes require documentation.

Bouchet (2006) also expresses concern that the desire of users for stability inhibits the introduction of necessary and justified change; can users impose a "tyranny" that must be

resisted if progress is to be made? It is true that users desire stability; few, however, are likely to insist on it at all costs. Within the European fauna, segregations of very closely related species have been accepted without complaint, and the new (or old, but resuscitated) names have been incorporated into national and local checklists. A particular irritation for users arises when both generic and trivial names are altered, without the integrity of the taxon itself being questioned. Trivial names alter as a result, usually, of differing local usages resolved by priority; generic alterations appear to many users to be whimsical, reflecting a new, and often short-lived phylogenetic hypothesis. Changes of this sort promote confusion; the classic paper on the ecology of British land molluscs (Boycott, 1934) still holds valuable insights, but is impenetrable to the uninitiated because of nomenclatural changes since. Of course, many of these changes are justified, but users are right to resent repeated changes at generic level. We consider problems of supraspecific nomenclature further below.

Bank et al. (2006) cite many European authors who have accepted the CLECOM checklist as the basis of their nomenclature. However, others have not, for example Wiktor (2004) for the Polish fauna, or Cameron (2003) for the British and Irish fauna. The new British and Irish checklist (Anderson, 2005) similarly adopts some, but not all of the changes introduced in CLECOM. Thus, it is clear that the aim of a common list (of course, subject to revision over time) has not been achieved, because basic disagreements between systematists, rather than between systematists and users, have not been resolved. We fear that some users adopt the list assuming it to be authoritative, aware of their own lack of expertise in taxonomic matters, but unaware of the disagreements that still exist among specialists.

In practical terms, we acknowledge that it is probably impossible to get all specialists on a regional fauna to agree, unreservedly, to a standard checklist. An extensive consultation beforehand, and a willingness to list areas of ambiguity or disagreement would increase the chances of "buy-in" by the professional community as a whole. The three of us engaged in systematic research (BMP, AR and AW) were not consulted in their areas of expertise. Areas of ambiguity and disagreement would also be minimised by omitting some non-mandatory levels in the hierarchy of classification.

SYSTEMATICS AND TAXONOMIC JUDGMENTS

As mentioned earlier, the debate over these checklists reflects a wider concern with systematics generally (Godfray & Knapp, 2004), prompted partly by the increasing volume of molecular studies, and partly by the parlous state of funding and respect for traditional alpha and revising taxonomy (Wheeler, 2004). It is clear, also, that a concern for the consequences in conservation policies is involved (Bouchet, 2006; Davis, 2004). It seems to us that the participants have, to some extent, talked past each other. CLECOM is not the "Nouvelle École" resurrected, nor does Davis give undisputed primacy to molecular data. Rather, the debate is between those who wish to recognise as much geographical variation, and as much detail of currently accepted phylogeny in nomenclature as possible, and those who prefer a more conservative approach, requiring more, and more conclusive evidence before making nomenclatural changes. Davis' approach to these problems is essentially that of the population biologist; populations of a species vary. The variation may be interesting in itself, but before it acquires nomenclatural significance, some marked discontinuities must be shown, and shown to be genetic. By inference (since the data are not presented in detail), the CLECOM approach is more typological, because although, no doubt, several (or many) specimens are examined, revision proceeds on the basis of perceived non-overlap in characters between some allopatric populations, without evidence of the range of variation to be found within each designated taxon being presented. Again, this distinction is not dependent on the use of molecular data; we again refer to Solem's monographs (Cameron et al., 2005), in which copious morphometric and anatomical data are presented and analysed. It could be said, in legal parlance, that CLECOM goes for "the balance of probabilities", Davis for "beyond all reasonable doubt".

In this particular respect, taxonomy differs from nearly all other aspects of biological science. A disproved theory elsewhere can simply disappear from view (other than for historians of the science). In taxonomy, nomenclatural acts become enshrined in the literature. Revision of a group is an arduous task of surveying early literature and searching out of type material, made harder by the accumulation of new or badly documented names.

Changes should be very well supported and documented, and we suggest that there are many occasions when the possibility that an existing classification is wrong could be raised in publication without rushing to change nomenclature. Given the "debris" of erroneous classifications, Davis' stance seems to us to be the more prudent.

All participants in this debate adhere, in principle, to the biological species concept. All accept, in principle, that taxa at any level should be monophyletic; we are all cladists now. All recognise that, in a majority of cases, current nomenclature neither rests on direct evidence of reproductive isolation, nor on molecular data alone. Species as described are essentially morphospecies (Eldredge & Cracraft, 1980). In this context, we draw attention to a significant feature of terrestrial gastropods: self-fertilization is widespread, and inadequately researched and documented (but see Jordaens et al., 1998, 2000, 2001; Reise et al., 2001; Nicklas & Hoffman, 1981). Where it is the norm, the biological species concept does not apply, and, inevitably, nomenclature rests on the degrees of difference between populations or forms, whether morphological or molecular. This feature of terrestrial molluscs may well lead to the conclusion that some sister species, living in sympatry, are in fact ecophenotypic variants of the same stock. The complexities of such situations can best be explored by molecular analysis (Armbruster, 1997).

It could be said that convergence and parallelism are the curses of cladistic and phylogenetic taxonomy (in contrast, they are blessings for the evolutionary biologist). We might add ecophenotypic variation as an additional burden for the taxonomist to bear; both some shell and some anatomical characters are known to vary substantially with environmental conditions and seasons (e.g., Solem & Christensen, 1984). While molecular techniques, even with the very sophisticated analytical software available, are not the only weapon in the modern taxonomist's armoury, they have been used, elegantly, to reveal the convergent nature of morphological characters previously believed to be apomorphies (Gittenberger & Schilthuizen, 1996). An even more complex picture is obtained in a recent study of *Mastus* species (Parmakelis et al., 2003), with molecular variation suggesting many episodes of population mixing, whereas morphology suggests strong selection varying with locality.

While species, therefore, are generally rec-

ognised to be real entities, their definition is frequently, of necessity, more arbitrary. For taxa above this rank, the only rule that commands general assent is that, to the best of our knowledge, they should be monophyletic. Demonstration that they are not is an accepted ground for revision. A quick survey of higher taxa used in European terrestrial and freshwater malacology reveals just how unstable and contentious they are. Convergence and parallelism are very frequent (Davis, 1979; Wiktor, 1984; Pokryszko, 1994), and differing suites of characters can yield different conclusions. We certainly agree with Bank et al. (2006) that molecular data are not the only source of reliability. A field badly neglected in non-marine molluscan taxonomy is ontogeny, perhaps because it is more demanding of time and patience than other sources. We know the details of life history for only a minute proportion of the European fauna, and there appears to be only one case in which patterns of development have been polarised for cladistic analysis (teeth in the shell-mouth; Pokryszko, 1997).

Given the unreliability or ambiguity of many characters used to define higher taxa, it seems prudent to go for relatively few, large and inclusive taxa that have a greater probability of remaining monophyletic in the face of new research, rather than attempting, through nomenclature, to capture virtually every branch point within a clade. The more subdivision, the more opportunities there are for error. This is not to deny the interest of detailed cladistic studies at family level and below, which have revealed patterns of evolutionary divergence that can be related to environmental history (e.g., Pokryszko, 1997; Douris et al., 1998; Wiktor, 2000). We are not persuaded that all steps in such radiations need to be entrenched in nomenclature. Biogeographical and evolutionary studies are based on monographs and cladograms, not checklists and names.

Another issue in the debate concerns subspecies. Classically, subspecies are seen as distinctive, geographically replacing forms of a species (O'Brien & Mayr, 1991). In practice, allopatric populations, with no contact, but definable differences, are given subspecific rank when the differences are not thought to be sufficient to justify species status, but the group as a whole is believed to be monophyletic. While we agree with Davis, and, indeed, with Bank et al. (2006), that some listed subspecies in checklists are dubious entities, their total elimination from all checklists seems un-

warranted. Some may well turn out on further study to be worthy of species status, and reliable records of subspecies can then be allocated appropriately. It is worth noting, however, that if the term has been correctly applied (to allopatric or parapatric forms in a monophyletic group), recorders and naturalists will tend to name forms on their location, rather than on their characters, perhaps generating a spurious repeatability and coherence. In particular cases, where only one subspecies is known in the region for which a list is compiled, the subspecific epithet can well be omitted (Anderson, 2005). Given the very variable quality of subspecific designations, it seems rash to use the category as the basic unit of recording. That CLECOM I does so reflects a deep concern with the effect of nomenclature on conservation policies, the issue on which its authors and Davis disagree most strongly.

CHECKLISTS, TAXONOMY AND CONSERVATION

Davis (2004) singles out for criticism the explicit statements in Falkner et al. (2001) and in Bouchet (2002) supporting the use of subspecific names, even where the evidence does not unequivocally comply with the conventional definition of that rank, on the grounds that named entities are necessary if conservation campaigning is to be successful, and conservation policy to be implemented. There is much pragmatic force in this argument; names reify, creating a unique object for consideration. Bouchet (2006) emphasises this point, learnt by long experience in fighting battles for conservation. The complex, often mathematical, descriptions of within and between population variation that characterise the evolutionary, population genetics approach do not lend themselves easily to sharp campaigning or to legislative decisions.

Nevertheless, we agree with Davis that this is a distortion of taxonomic practice; in his words "... it risks to reduce taxonomy to a mere artifice possibly with grim consequences". The consequences Davis mentions are concerned with an explosion of badly supported names, confusion and taxonomic inflation. We see a significant further danger. Rightly, Bouchet (2006) says that we do not operate in a socio-logical vacuum. But we do not operate in a legal or political vacuum either. Those whose interests are threatened by specific conservation measures will quickly exploit a perceived

inflation of taxonomic entities. Forensic examination of expert witnesses, and of the assumptions on which their conclusions were based, would reveal the underlying confusion, making the case for conservation harder. If the science underlying one nomenclatural decision is found wanting, other such decisions based on better evidence will share in the discredit. Other arguments, however difficult, should be used to advance conservation policies, and we should stick to strictly scientific criteria for nomenclatural decisions.

Despite our criticisms of the CLECOM list as it stands, we certainly support the intentions of the authors. For reasons given at the start, Europe has particular need of an agreed list of land and freshwater molluscs. The fact that the list has initiated a fierce debate within and outside that continent indicates its heuristic value. That it has exposed differences of view both about taxonomic practice, and about the functions and form of checklists can only be beneficial.

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